Technical Peer Review of the
NSTX Five-Year Program Plan

Princeton Plasma Physics Laboratory
June 30 – July 2, 2003

Summary Report Prepared by Stephen Eckstrand (DOE/OFES)

Executive Summary

Princeton Plasma Physics Laboratory (PPPL) submitted a Five-Year Proposal entitled *NSTX Five Year Program Plan* for the period of October 1, 2003, thru September 30, 2008. The total cost of the proposal is $199,177,000. The NSTX team began to formulate the technical component of the proposed program plan in 2002, beginning with a five-year plan opportunities forum in June 2002. The team then prepared a draft five-year plan, which they presented to the NSTX Program Advisory Committee for further advice about the program. The final plan was submitted to the Office of Fusion Energy Sciences (OFES) on June 16, 2003.

The Office of Fusion Energy Sciences conducted a technical review of the proposed NSTX Five-Year Research Program at the Princeton Plasma Physics Laboratory on June 30-July 2, 2003 with a nine-member, international review panel consisting of B. Hooper (LLNL - Chair), R. Bravenec (Univ. of Texas), D. Hill (LLNL), F. Jaeger (ORNL), E. Marmar (MIT), D. Post (LANL), T. Strait (GA), A. Sykes (UKAEA), and Y. Takase (Univ. of Tokyo).

The charge letter to the review panel is included in Attachment 1. In this charge letter, OFES asked the panel to assess the **importance and relevance** of the proposed 5-year research program with respect to the goals of the U.S. fusion program, the **scientific and technical merit** of the ongoing and planned research, the **competency** of the proposed senior research personnel, the **adequacy** of the proposed resources, and current level of **performance** of facility operations.

The NSTX team members made presentations to address the questions in the charge letter during the first 1.5 days of the review, and the panel discussed technical issues and formulated initial feedback to the NSTX team during the remaining day. The NSTX Five-Year Plan document and the Five-Year Plan Presentations are available at the following web-site:

http://nstx.pppl.gov/Pages_folder/research_folder/5YrPlan.html

The panel members provided answers to the five questions in the charge and commented on the topical areas that were covered in the presentations at the review. Answers to the five questions are summarized in the remainder of this Executive Summary. More details on the five questions in the charge as well as comments on the technical issues are included in the Details and Technical Comments section.

The NSTX 5-Year Objective is to make a preliminary determination of the attractiveness of the spherical torus (ST), by assessing high-beta stability, confinement, self-consistent high-bootstrap operation, and acceptable divertor heat flux, for pulse lengths much longer than an energy confinement time. With respect to importance and relevance, the review panel concluded that proposed program is clearly congruent with the Integrated Program Planning Activity (IPPA) Goal 2 and is well positioned to meet the 5-year objective of a preliminary evaluation of the attractiveness of the spherical torus (ST) concept about mid-way through the 5-year planning period. The panel members agreed that the outstanding scientific issues for the ST were clearly identified and addressed in the 5-year plan. These issues include MHD equilibrium and stability, transport, and the scientific basis for startup without a central solenoid and non-inductive current drive. They also noted that NSTX research is well coordinated with other innovative confinement concept research, particularly with that on other STs, both in the U.S. and abroad.

On the question of scientific merit, the panel members were very impressed by the quality of the science and commended the NSTX team for their technical achievements. Several panel members commented on the significant breadth and depth of the proposed NSTX research program. They also stated that NSTX has made remarkable progress during its first 5 years of operation, culminating with the achievement of 35% toroidal beta. There was general agreement that NSTX is at the forefront of ST fusion research and is establishing the U.S. as one of the world leaders in this confinement geometry.

Concerning competency, the panel members indicated that the senior PPPL and non-PPPL management personnel are highly competent and that the NSTX team, both PPPL staff and collaborators, are an excellent group of scientists and engineers, who are doing a good job of carrying out the research program. They also noted that PPPL has assembled an excellent group of international collaborators. Finally, they observed that the NSTX team had assembled a competent and effective advisory committee to provide oversight and guidance to the program.

The panel concluded that that the proposed facility upgrades and diagnostics, along with the ongoing interactions with theorists and with outside collaborators, are fully adequate to carry out the proposed research. However, they noted that the requested funding is approximately 10% above the FY 2004 request and that if the requested budget is not available, the rate of progress will be slowed and there will be more scientific risk.

With respect to facility operations, the panel members found that the project completed its important milestones close to (or ahead) of schedule, through the end of 2002. They also noted that the integration of on-site collaborators into operation is thorough and effective. Further, they affirmed that the NSTX project has excellent safety record and is working to be even safer. However, they concluded that a toroidal field coil failure in early 2003 has cost about 8 weeks of run time this fiscal year, delayed the start of research in FY 2004, and required the diversion of engineering and financial resources that could otherwise have been used to implement upgrades and diagnostic improvements. Finally, they concluded that the project has responded to the failure in a thorough and professional manner, including convening a panel of outside experts to review the design for repair.

Details and Technical Comments

1. Importance and relevance of the proposed research to the U.S. and world fusion programs:

The NSTX 5-Year Objective is to make a preliminary determination of the attractiveness of the spherical torus (ST), by assessing high-beta stability, confinement, self-consistent high-bootstrap operation, and acceptable divertor heat flux, for $\tau_{pulse} >> \tau_E$. The research program to achieve this objective is well aligned with the IPPA goals for the spherical torus, including the issues of MHD equilibrium and stability, transport, and non-inductive current drive and startup without a central solenoid. The members of the review panel concluded that the NSTX team has an aggressive program in all of these areas and has already made substantial progress toward the IPPA 5-year objectives in each of these areas, including the achievement of $\beta_T \approx 35\%$, $\chi_i \approx$ neoclassical, $\chi_e \approx 10\ m^2/s$, $I_{non\text{-}inductive} \approx 60\%$ and $I_{bootstrap} \approx 50\%$. Overall, the panel members also agreed that the NSTX team has compiled an impressive set of results during the initial years of operation.

The panel members generally agreed that the proposed NSTX program would make important contributions to fusion research, both within the US and worldwide. They noted that the program is clearly addressing the goals of the US fusion program, in particular the IPPA goal 2 on innovative magnetic confinement configurations. The NSTX team has identified the outstanding scientific issues for the ST configuration and has made plans to address them within the next 5 years or to implement the facility upgrades that will be necessary to do so in the subsequent 5-year period. Overall, the panel members felt that the primary areas of challenge for the next 5 years will include transport (particularly electron transport) and the scientific basis for non-inductive current sustainment, profile control, and startup without the use of the central solenoid.

The review panel members observed that much of the proposed research is also important and relevant to the ITPA and to tokamak research in general. Comparison with tokamak data allows critical tests of theoretical models and empirical scalings related to stability, transport, and fast ion effects, potentially yielding a much clearer distinction between competing models. In addition, there is a good opportunity to study electron transport physics on NSTX, where χ_e is relatively large and it may be possible to suppress long wavelength modes. This would enable specific studies of short-wavelength (ETG) modes, which would be a major contribution to the physics of tokamak turbulence. On the other hand, the committee members stated that it is also important to establish that τ_E extrapolates to acceptably large levels for future, larger ST experiments.

The panel members found that research on NSTX is well coordinated with national and international fusion research, including collaborations with tokamaks as well as other

STs, particularly with MAST. An example of this coordination was the recent visit to MAST by an NSTX researcher to study Electron Bernstein Wave (EBW) heating and current drive. However, they noted that increased interaction with other STs, including Pegasus and other smaller facilities around the world should be encouraged. The proposed EBW research on NSTX may be useful for heating and current drive in low magnetic field Innovative Confinement Concepts, such as the RFP and Spheromak. Collaborations are underway or being considered with CDX-U, HIT-2, Pegasus, and other STs around the world. These collaborations could both strengthen the research programs of the smaller STs by supporting their work and provide physics and technology guidance to the NSTX team.

The committee believes that proposed upgrades to the machine (diagnostics, EBW, etc.) will prepare the NSTX facility for a program that can be extended beyond 5 years. However, given that funding may be limited to less than the budget request, it is probably a reasonable strategy for the NSTX team to rely on MAST (Culham) to address detailed divertor physics studies, except for those issues that are important for NSTX operation at the highest planned power levels. Several committee members felt that the proposed program is, in general, more than is needed to meet the IPPA objectives, and given the likely budgets and resource levels (including operating time), the NSTX team will need to make a strong effort to prioritize their goals and program if they are to deliver on their major goals.

2. Scientific and technical merit of the proposed research:

The committee members were unanimous in their conclusion that the proposed research is of high scientific and technical quality. It addresses many key physics issues for development of advanced tokamaks and STs, and builds on the previous successes of the NSTX experiment. They felt that the proposed research is well thought out, and is well supported by theory and numerical modeling. They concluded that NSTX is clearly at the forefront of fusion research, and establishes the US as a world leader in this confinement geometry.

The review panel members affirmed that the proposed facility upgrades and diagnostics, along with the interactions with theory and with collaborators, are fully adequate to carry out the proposed research. However, they cautioned that if the requested budget is not available, the rate of progress will be slowed, and there will be more scientific risk. They indicated that the EBW heating and current drive and the MSE diagnostic are critical to the success of the proposed plan and will need adequate resources to maximize their success.

While the NSTX program has the potential to make important contributions to tokamak research in general, many of the issues are specific to STs. In particular, a few panel members noted that it is difficult to see how many of the current drive startup scenarios, the EBW current drive system, the benefits of the very high degree of shaping, the relatively large ratio of gyroradius to system size and other benefits of very small aspect ratio can be exploited by a more conventional tokamak burning plasma experiment or by a fusion power plant, both of which will be limited to aspect ratios of 2.5 or so by the need to provide space and shielding for the PF, the OH solenoid, and TF inner superconducting coils.

a) MHD Stability

The panel members commended the NSTX team for the remarkable progress in the stability area, culminating in the achievement of 35% toroidal beta within the first 5 years of operation. They commented that MHD is an area where there has been strong and successful participation by collaborators, particularly the work being done by the Columbia University group.

The planned research in the MHD area is aimed at two broad goals: 1) achieving stable operation at high beta with a high fraction of the plasma current carried by the bootstrap current and 2) extending our understanding MHD science through exploration of unique dimensionless parameter regimes.

The panel members generally agreed that the planned research on the high beta goal appears appropriate. They indicated that the addition of external, non-axisymmetric control coils is important, both for control of resistive wall modes and reduction of error fields, and strong shaping (elongation and triangularity) will be an important tool for reaching the NSTX high beta goals. Error fields, which have already been significantly reduced, may still be playing an important role in limiting high performance; a weak scaling of confinement with increasing plasma current may be a clue in this regard. In addition, modeling predicts that the external coils may be capable of direct feedback stabilization of resistive wall modes in the absence of rotation, and confirmation of this would be an important benchmark of the models. Finally, the panel members commented that rapid implementation of external control coils to investigate, and possibly address, this question will be important.

The panel members pointed out that second MHD goal, to extend scientific understanding of MHD, presents many exciting opportunities (e.g. ELMs, 1/1 modes, fast-ion modes, etc.). These topics have much scientific merit and are important for burning plasmas. The planned emphasis on the effects of plasma flow, flow shear, and error field effects is also important, given the relatively high rate of rotation typical of NSTX. Planned work by the PPPL theory group to include sheared rotation in MHD equilibrium and stability codes is important to NSTX as well as other existing and planned experiments. However, the panel recognized that the NSTX team has a very ambitious plan, and budget realities may force a narrowing of the scope.

The panel members also noted that stabilization of neoclassical tearing modes through localized EBW current drive could be very important, but is uncertain. This method of current drive, with the necessary spatial localization, remains to be demonstrated. Avoidance of neoclassical tearing modes through tailoring of the global q-profile is an alternative.

Many of the planned facility upgrades and new diagnostics are critical to the MHD stability portion of the plan. In particular, measurements of the current density profile are very important for MHD studies, and the MSE systems should be brought on line as quickly as possible. The ST has an advantage, relative to conventional aspect ratio tokamaks, with regard to poloidal field measurements, because at low aspect ratio, the poloidal and toroidal field strengths are comparable, at least in the outer half of the plasma. However, for MSE, this advantage is probably more than offset by the difficulties of making this measurement with small absolute field strength, which in turn means that the stark splitting is very small. Two approaches to MSE are being investigated. Nevertheless, some panel members concluded that MSE at 0.4 Tesla is likely to prove very difficult, and alternatives should be considered.

Finally, a panel member noted that given the low toroidal field in NSTX, the role of finite Larmor radius stabilization needs more study. This is important in sorting out the physics that results in the high betas and affects the extrapolation to higher field, next generation STs.

b) Transport and Turbulence

The proposed work on turbulence and transport is a critical element of the NSTX program. The panel members stated that detailed studies of transport coefficient profiles should be pursued vigorously. However, they noted that global scalings, with both dimensionless and engineering parameters, should not be ignored, since these can be very useful when comparing across devices and in extrapolating to next steps and reactors. NSTX should be particularly well positioned to help answer questions concerning confinement scaling with aspect ratio. The panel pointed out that it is important to measure the global confinement times consistently with ITPA guidelines. A unique suggestion was that an effort should be made to try to fit NSTX data to existing scaling laws by adjusting the definitions of parameters such as q (*e.g.,* q_{eng} versus q_{95}). This might have a minor effect on data from conventional tokamaks, but a large effect for STs, thereby bringing the NSTX data in line. The scaling of confinement time with I_p and input power is particularly important for extrapolation to reactors.

The panel members noticed that the NSTX H-Mode appears to be somewhat different from H-mode in larger aspect-ratio tokamaks. In particular, while pedestal and ELM phenomena may be qualitatively similar, the global energy confinement does not increase significantly in H-Mode when compared with L-mode. As a result, detailed studies of pedestal physics and edge turbulence could prove to be especially illuminating, and edge turbulence measurements should be compared with predictions of various theories and models. The connection with electron channel transport may also be relevant here. Some panel members were of the opinion that transport barrier and edge pedestal physics is the most important area for transport studies and encouraged the NSTX team to spend more effort on this area. The panel pointed out that planned diagnostic upgrades for transport, such as the high resolution Thomson scattering system to measure edge pedestal conditions, are important for the success of the transport studies.

The review panel members also encouraged the NSTX team to continue their efforts to experimentally verify their apparent ability to turn off low-k turbulence, but noted that this remains to be verified experimentally with low-k turbulence diagnostics. The panel also encouraged enhanced theoretical work in this area (e.g. refined neoclassical and nonlinear gyrokinetic calculations).

The panel members stated that the electron-channel dominated transport regimes on NSTX may provide a unique opportunity to study the electron transport channel, and priority should be given to the planned short wavelength turbulence diagnostics, along with the associated theory and modeling. The potential of microwave imaging diagnostics to provide 2-D imaging is exciting and might lead to simultaneous measurements of low-k density and electron temperature fluctuations. Nonlinear low-k gyrokinetic simulations should proceed and the results should be compared with experiment. Apparent "Electron Temperature Gradient-only" (ETG) transport is unique in fusion research experiments. The panel encouraged the NSTX team to continue its efforts to verify the existence of ETG transport experimentally (high-k turbulence diagnostics) and theoretically (nonlinear gyrokinetic calculations). They recommended that nonlinear ETG simulations should be initiated for comparison with experimental results.

c) Heating and Current Drive

The review panel members concluded that non-inductive current drive is very important for proof of principal tests on NSTX. The NSTX team has identified Electron Bernstein Wave Current Drive (EBWCD) as the best option to use. The panel recognized that this approach is speculative, but concluded that the experiments are important and should be pursued aggressively. They cautioned that development of high power tubes in the required 15 GHz frequency range will certainly take a long time, and be expensive. Furthermore, the tube development may be delayed due to technical or financial difficulties, and thus any alternatives to allow for earlier experiments, if they exist, should be considered in order to validate predictions. The MAST results with EBWCD are at a non-optimum frequency (60 GHz) to date, but are of considerable interest to NSTX. There should also be useful information from 8.2 GHz experiments on TST-2 and several other STs.

The other current drive technique being explored by the NSTX team is High Harmonic Fast Wave (HHFW). The panel noted that the NSTX team recognizes the important issues and their program plan addresses them. The issues include:

- Does HHFW always heat as assumed in the integrated modeling?
- Are there cases in which power absorption is not 100%, or the power split between electron absorption and ion absorption is inconsistent with theoretical predictions?
- What do these imply about the ability to rely on this technique? Is there enough confidence that it can be used for p(r) control?

The panel members concluded that the implemented hardware improvements should increase the reliability for experimental investigations. Consideration of effects such as edge absorption should also be included in the work.

d) Solenoid-Free Startup

Demonstrating solenoid-free startup is recognized by the NSTX team as a very high priority topic, and is crucial for the future of the ST as a candidate for a component test facility or a reactor. The panel members found that the main technique applied so far, Coaxial Helicity Injection (CHI), is very interesting from the point of view of scientific investigation, but has so far not shown much promise as a practical start-up technique and faces challenges for extrapolation to larger systems. In particular, it is not clear that the short-pulse CHI method is any better than using pre-ionization and a conventional null. The proposed FY04 tests on NSTX have the potential to clarify this, and the panel supported the NSTX efforts to give this a high priority.

Given the challenges of CHI, the panel members felt that it is important to investigate additional approaches to non-solenoidal startup. Such methods as EBW, Compact Toroid Injection, etc., are speculative and cannot be quickly tested. Poloidal Field ramp-up, however, is well understood and predicted to be especially effective at low aspect ratio. The schemes for poloidal field ramp-up outlined by the NSTX team should be tested on NSTX as soon as feasible. The presence and persistence of field null can be tested by direct measurement (even without the toroidal field.). The panel recommended that the NSTX team consider using a capacitor bank to provide the initial small positive current in PF5 coil.

e) Boundary Physics

The NSTX boundary physics group proposes to carry out fundamental research on the boundary plasma including understanding peak divertor heat loads, edge-plasma transport, and particle/impurity control. These are significant problems for any high power toroidal confinement device (e.g., a tokamak) and pose special challenges for STs. The ST concept poses a number of interesting and challenging boundary physics questions that must be addressed to show the ultimate potential of the concept as a reactor or a CTF. Some reviewers expressed the concern that while it appears that much of the proposed research does touch on these topics, the explicit connection to advancing the concept was not articulated clearly. Other review panel members noted that the NSTX team is not so large in this area, and should probably concentrate its efforts on the highest priority topics (i.e. those needed for successful operation of NSTX at high power density). Areas for concentration on NSTX should include power handling, particle control and pumping, and H-Mode ELM and pedestal physics. The gas puff imaging edge fluctuation experiments are leading the world effort in this area, and can be expected to continue to make important contributions. Some other topics might be better studied through collaboration, for example with MAST.

The panel felt that the focus on particle control is necessary to optimize non-inductive current drive and that lithium surface conditioning appears to be a cost effective way to reduce recycling. Divertor pumping would be relevant to potential future high-power long pulse ST devices, but the cost may be prohibitive. The NSTX team stated that the expected heat fluxes would be acceptable for the planned pulse lengths; however, some panel members cautioned that this may not be the case if they actually succeed in getting ~10 MW of heating and CD for 5 seconds in a highly elongated, high-triangularity plasma. They suggested that plasma shapes compatible with heat flux reduction techniques (e.g. double null vs. single null experiments) be explored before carrying our high power experiments.

The panel members observed that the study of edge-plasma transport (convective vs. diffusive or intermittent) is currently an important topic in boundary physics research. The NSTX team has proposed expanded use of turbulence diagnostics to measure edge fluctuations, which will provide relevant new data that can be compared with edge models such as BOUT. The proposed edge-pedestal studies will require increased spatial resolution for edge diagnostics, such as the planned Thomson scattering system upgrade. The panel members commented that research in this area is just beginning but is important, as differences in pedestal behavior may be linked to differences in ST confinement scaling.

The panel members stated that the proposed efforts for improving plasma fueling would be strengthened if there were numerical studies showing how various fueling profiles might optimize high beta, non-inductive CD scenarios. Such studies could be useful in deciding what sorts of new fueling methods to pursue and would strengthen the presently somewhat unclear physics basis for their strategy.

The reviewers cautioned that while the lithium divertor studies are very interesting and promising, they will have a major impact on the machine. In particular, a lithium divertor will require significant design and installation time, and a lot of run time. The proposed Li divertor program by itself could take as long as two years, and possibly more depending on how long it takes to install and learn how to operate a lithium divertor.

f) Control and Integration

The committee recognized that NSTX control and integration goals are ambitious, leading to quasi-steady-state, fully non-inductive, high beta plasma scenarios. However, they commented that the proposed 5 year plan is somewhat vague in the description of the requirements for control systems and feedback algorithms. The descriptions are all qualitative, and so it is difficult to judge if the actual requirements have been carefully considered. A few examples of this are some quotations from the plan: "response is adequate ... provided elongation is not too high and l_i is not too low ... future experiments will demand greater capability;" "requirements to stabilize RWM's are not yet known ... required response time will be considerably shorter than that needed for axisymmetric position control ... should still be within capabilities of present system;" and "faster response than is available from the existing phase-controlled rectifiers and their communication link will be needed." The reviewers recommended that these requirements be quantified, so that explicit hardware and software needs can be identified.

Overall, the modeling needed to design controllers is at an early stage, and needs further effort, particularly for the non-solenoidal start-up scenarios. The panel members recommended that the NSTX team consider the use of diamagnetic flux (as on DIII-D) rather than real-time EFIT as a first step for control beta. This is not as powerful as the profile approach, but it should be much easier to implement, especially given the challenge of current profile measurements for NSTX.

g) Integrated Scenario Modeling

The NSTX team has begun a serious effort to utilize integrated scenario simulation. The panel endorsed the NSTX team vision that integrated scenario modeling has the promise to greatly improve operational efficiency, and optimize the selection of experiments. The panel members felt that the NSTX team's approach of constraining many of the unknowns with experimental data (i.e. basing the models on TRANSP base cases) is reasonable. This avoids the necessity of inventing ad-hoc density transport models, for instance, that reproduce the experimental density profiles. The NSTX team also recognizes that free-boundary calculations that include the interaction with PF system are important for CD scenario selection, and that is reflected in their modeling approach.

While the panel members commented that the NSTX team has made a good start with their TSC/TRANSP modeling effort, they recommend that the team put more emphasis on self-consistent integration of current drive calculations with TSC. This would greatly improve the efficiency of scenario simulation, since the link between the packages is currently done laboriously by hand. The panel also supports the efforts to include rotation effects in the MHD equilibrium. The panel also observed that, although an initial effort has begun, the effort would proceed much faster if the one person working on this were given more help to speedup the calculations.

Theory and Modeling

In general, the panel members felt that the theory and modeling group is doing a good job in beginning to address the unique scientific issues in NSTX. High toroidal rotation velocity, flow shear, and rotational damping are very important in NSTX and, therefore, should be given highest priority for inclusion in the models. However, a few panel members noted that the present modeling of Electron Bernstein Wave (EBW) Modeling with GENRAY does not account for mode conversion of the electromagnetic waves to EBW. One option might be an all-orders, full-wave model in 1-D including a poloidal magnetic field and parallel magnetic field gradients. Such codes have been developed in the SciDAC Project on Electromagnetic Wave-plasma Interactions.

Given the NSTX 5-year objective is to determine the attractiveness of the spherical torus as a fusion device, some reviewers felt that there should be more emphasis on modeling next step STs. In particular, next step STs may not be in the high harmonic fast wave regime; therefore, modeling of these devices should include conventional mode conversion from the fast wave to the IBW or ion cyclotron wave. This could give off-axis heating as seen in Alcator C-mod and possibly off-axis current drive.

The panel was somewhat confused by plans calling for incorporation of transport coefficients derived from gyrokinetic simulations into predictive analysis codes such as TRANSP in FY03-04. After clarification from the modeling group, it was understood that this means "simplified global" transport coefficients that are approximate fits to data from the gyrokinetic codes. This needs to be planned carefully; otherwise the computer time needed for first results will be prohibitive. The presentations listed a large number of codes and theorists who were working on NSTX. While using almost every code in the fusion program has some sociological advantages, more focus would be appropriate and more effective.

A few reviewers pointed out that the support of the PPPL theory group for NSTX could be stronger. The question of balance between support of NSTX and the stellarator effort at PPPL should be assessed again.

3. Competency of Proposed Senior Research Personnel

The review panel was unanimous in its finding that NSTX team, including major collaborators, is world-class. The senior personnel are highly competent, and are doing a good job of guiding the research. It is clearly a major challenge to coordinate the efforts of the large NSTX team and to operate the experiment as effectively as they have done. Senior management is involved in the day-to-day NSTX operations and strongly participated in every part of the review. The panel members commented on the quality of the PPPL theory group and that the theory group's work on ST theory seems well coordinated with the experiment.

A sign of health for the program is that many of the presentations were given by "the next generation." The future leaders for the fusion program are being prepared. However, the panel was somewhat surprised that there are only three graduate students involved in the program. Several reviewers recommend that NSTX should continue outreach to increase the number of graduate students involved in the experiment.

Overall the panel members felt that the NSTX team has successfully applied the TFTR capabilities — power supplies, site location, and general infrastructure— to the experiment, yielding capabilities that considerably exceed what otherwise would have been possible at the NSTX construction cost. The NSTX team has built a successful national and international collaboration program, and the core group of collaborators who spend a large fraction of their time at PPPL are well integrated into the research program. Numerous off-site collaborators are also effective and significant contributors.

4. Proposed Costs and Budget

The review panel concluded that the proposed total costs (a 10% increase over the FY 2004 President's request) appear to be reasonable and approximately adequate for the proposed scope. The NSTX team has identified a reasonable prioritization of tasks, along with areas to be slowed or postponed in case available budgets are lower than those in the plan. The balance among hardware and diagnostic upgrades also appears to be reasonable, although the scope of the boundary physics diagnostic upgrades appears to be very ambitious, given the size of the research group in this area. While proposing an expanded program with increased funding, the NSTX team clearly recognizes budget realities and has tentatively identified a strategy to continue moving forward, but at a reduced pace with increased scientific risks due to necessary focusing of efforts. The proposed trade-offs between run time, upgrades to the facility, and diagnostics seem reasonable at the Presidential request level. If further reductions are mandated (below the President's budget), the impact on EBW physics and non-solenoidal startup should be minimized, possibly through expanded collaborations and modeling in these and other areas. These research topics are unique to the ST and critical to the application of the concept. In the diagnostic area, MSE or alternative(s) to measure current density profile should get priority.

5. Management of Facility Operations

The review panel commended the NSTX project for completing their important milestones, close to, or in some cases ahead of, schedule, through FY02. However, the panel members noted that the TF coil accident has cost about 8 weeks of run time in FY 2003 and delayed the start of research in FY 2004 and that the cost of repairs has clearly had an impact implementation of upgrades and new diagnostics. Several panel members noted that the accident was a design flaw that could and should have been avoided. Nevertheless, the NSTX project has responded to the coil failure in a thorough and professional manner; the approach to having the TF redesign reviewed by outside experts is completely appropriate. Several panel members commended the practice of using outside experts on important design review panels.

The panel concluded that for the long-term success of the program, it is essential that the facility be returned to operation with the capability to operate, with high confidence, over the full range of fields required for the planned scientific program. Certifying the design for operation at the original specification of 0.6 Tesla on-axis, would greatly enhance that confidence, even if most of the anticipated operation will be at somewhat lower fields. Additionally, other major systems should be reviewed to ensure that other failures are not lurking in the background.

The panel members noted that the project has an excellent safety record, and is working to be even safer. The integration of on-site collaborators into the operation is thorough and effective; in this regard, it is worth noting that adoption of MDSplus has contributed positively to this integration.

The staged upgrades to the diagnostic data systems to replace the CAMAC electronics with more modern electronics is very important, and the panel encouraged the NSTX team to carry out their planned program in this area

6. General Comments

A few reviewers noted a greatly improved sociological environment at PPPL with the NSTX team compared to the TFTR/PLT days. There appeared to be a much more constructive and collegial working environment, both among PPPL staff and between PPPL staff and those from other institutions. Without these changes, building a successful collaborative program on NSTX would have been much more challenging.

Attachment 1

To: Distribution

Subject: PPPL Proposal for 5-Year Research Program on the National Spherical Torus Experiment (NSTX)

Thank you for your willingness to participate in a technical review of a proposal from Princeton Plasma Physics Laboratory (PPPL) to continue research on NSTX for another 5 years, beginning on October 1, 2003.

As most of you are aware, the NSTX is the largest innovative confinement concept experiment in the United States. PPPL leads a national NSTX program that includes major collaborations with national laboratories, several universities, and private industry in addition to PPPL. Collaborators at these institutions receive about 30% of the NSTX research funding directly from the OFES. In addition, the NSTX program also includes several smaller scale collaborations with other U.S. and foreign laboratories and universities. PPPL has the overall responsibility to lead the NSTX research program, in close partnership with the collaborators, and to operate the NSTX facility.

The NSTX team is preparing a 5-year research and operations plan that will be provided to you directly from PPPL. This plan will cover the entire planned NSTX program, including work to be carried out by collaborators, but will concentrate on the PPPL scope of work. The plan will also contain appendices by all of the major collaborators participating in the NSTX Program, which will summarize their proposed scopes of work during the 2004-2008 time frame. PPPL will also provide you with detailed cost and schedule information for their proposed scope of work and a less detailed summary for the whole national program.

We are asking you to review the importance and relevance of the overall 5-year NSTX Research Program as described in the proposal and in the presentations that will be made to you. We are also asking you to evaluate both the scientific and technical merit of PPPL's scope of work and PPPL's operation of the facility. You do not need to review the scientific and technical merit of the research that will be carried out by the collaborators, since it is peer reviewed separately every 3 years. We would like you to perform the following assessments:

1. Assess the **importance and relevance** of the proposed 5-year research program with respect to the goals of the U.S. fusion program as outlined in the Integrated Program Planning Activity (IPPA), and in particular to the second goal: "Resolve outstanding scientific issues and establish reduced-cost paths to more attractive fusion energy systems by investigating a broad range of innovative magnetic confinement configurations." Is the research plan likely to accomplish the IPPA objectives? How well is the research coordinated with other national and international innovative confinement concept research activities? Also, where

applicable, please comment on the importance and relevance of the proposed NSTX program to the ITPA and tokamak physics in general.

2. Assess the **scientific and technical merit** of the ongoing and planned research. Does the research proposed address science issues at the forefront of the field? How well does the ongoing and planned research maintain a U.S. leadership position in key areas of fusion research? Are the proposed diagnostics, other facility upgrades, interactions with theory and modeling, and collaborations adequate to carry out the proposed research program?

3. Evaluate the **competency** of the proposed senior research personnel and the adequacy of the proposed resources. Assess the program's governance practices and the performance of the direct program management as well as the support provided from the host institution. How well qualified are the applicant's personnel to carry out the proposed research? Do the collaborative arrangements achieve the goal of an integrated NSTX research team?

4. Assess the reasonableness of the proposed **costs** for fusion **research** and **operations.** The cost review should be done at a summary type level, examining major items and projections from ongoing operational experience.

5. Assess the current level of **performance** of facility **operations**. Are milestones being met? Are planned operating, maintenance, repair and upgrade schedules being achieved? Are environment, safety, health and quality assurance matters being addressed appropriately?

As indicated above, these programs are carried out by collaborative national research teams. The proposed research plan from the national team should be reviewed for the importance and relevance of the proposed work to the U.S. fusion program and the adequacy of the proposed equipment to carry out that research. You are also welcome to comment on the relevance and importance of the research carried out by the major collaborators, summarized in the appendices. Please feel free to comment on any other issue relevant to the proposal.

Bick Hooper has kindly agreed to chair the review. He will run the meeting and will provide a brief oral summary at the conclusion of the review. The NSTX team will provide you with copies of the proposal, their presentations, and other material helpful for the review. I would like to receive individual written comments on your findings, in a brief draft outline at the conclusion of the review, and a written report by July 14, 2003.

The review will take place at PPPL on June 30-July 2, 2003. Please do not hesitate to contact me or Steve Eckstrand in my office if you have any questions.

John W. Willis
Director, Research Division